Q & A

Yuh-Nung Jan

Yuh-Nung Jan is at the University of California, San Francisco (UCSF). He studied Physics as an undergraduate at National Taiwan University. In 1968, he entered Caltech as a graduate student in theoretical high-energy physics. Two years later, he switched to biology. After he completed his Ph.D. with Max Delbrück in 1974, he joined Seymour Benzer's lab as a postdoctoral fellow. There he began working on Drosophila *neurobiology, together with his long-term collaborator (and wife) Lily Jan. Following a short second postdoc with Steve Kuffler at Harvard Medical School, they started their own little lab at UCSF in 1979 and have happily settled there ever since. He has worked on many areas of neurobiology, including peptide neurotransmitters, potassium channels, neuronal cell fate specification, asymmetric cell division and most recently, dendrite development.*

Why did you switch from physics to biology? In 1970, having been a student at one of the great centers for physics, I was already 23 years old and should have been reaching my peak as a theoretical physicist (why theoretical physicists tend to do their best work in their twenties is a very interesting mystery). If I were going to do something significant in physics, there should already have been some clear signs of it, but none was forthcoming. I was also very interested in biology in a naïve way. It was a stroke of luck that, even though I went to Caltech for its great physics department, it happened also to have a fabulous biology division which gave me my first exposure to modern biology. In the early 70s, biology was becoming very exciting, so I decided to switch.

Was the switch difficult? It wasn't too bad. One major reason was that Delbrück, my Ph.D. advisor, himself a physicist turned biologist, was very helpful and supportive of the transition. But there were some rough moments. Although I knew next to nothing about biology at the time of my switch, I was immediately assigned as a teaching assistant for one of the undergraduate biology courses. For a while, I must have been the worst biology teaching assistant Caltech ever had. Having to explain things to those extremely bright Caltech undergraduates who were more knowledgeable than I was to begin with forced me to learn really quickly.

What is the best advice you have been given? From Max Delbrück: don't do fashionable science.

What advice would you offer a prospective biologist? I would pass on Delbrück's advice not to do fashionable science. Perhaps it is useful to keep in mind that one way to judge a scientist is to do a 'gedanken loss-of-function experiment' (I believe this is how HHMI judges their investigators). Imagine that, instead of entering biology, scientist X had chosen a different career, say as a lawyer or a rabbi, how might biology have been affected? If there were no discernable effect, it would suggest the person's work is either non-essential or redundant. It would be very hard to make a distinct contribution in a very crowded field. Besides, it is no fun to have discovered something and then find out six other labs have done the same.

Do you have a favorite paper? "Mapping of behavior in *Drosophila* mosaics", a 1972 *Nature* paper by Hotta and Benzer. I was browsing through journals trying to figure out what to do after my Ph.D. and came upon this paper. It was so elegant and interesting. This paper inspired me to want to work with Benzer. What he was doing then was definitely neither mainstream nor fashionable.

What is your favourite conference? Like everyone else, I like small conferences with high quality science held at a nice place with interesting walks. One of my favourite places for conferences is Madrid: between sessions, I can go to the Prado and revisit their amazing collection of paintings by Velasquez, Goya and Bosch.

What was the best conference that you missed? There was a conference in France that I couldn't go at the last minute, so a postdoc substituted for me. He took the opportunity to meet up with his girlfriend (now wife) then living in Europe. The rendezvous must have gone splendidly well. As a result, a beautiful baby girl was born nine months later. Had I gone to the conference, this wonderful girl would not have been brought into this world. My missing the conference was definitely worthwhile.

Do you have any scientific heroes? If so, who and why? I was really fortunate to spend my formative years at Caltech. My scientific heros were my mentors and teachers from that era. Max Delbrück, with his unique combination of intellect, moral integrity and charisma, was a marvelous mentor. Richard Feynman, he was the smartest person I have ever met. I remember taking a couple of advanced physics courses taught by him. He was a mesmerizing lecturer. He made the subject look deceptively easy — until you tried to figure it out yourself. He was one of those extremely rare persons whose mind seems to operate at a level qualitatively different from the rest of us. Seymour Benzer, for the way he enjoys life and for being able to do really creative and original work consistently for six decades. At age 81, he is still doing and publishing work as interesting as ever. For us aging scientists of the baby-boom generation, this is very inspiring. The flip side is that my 'scientific heros' set impossibly high standards.

Do you have any strong view on journals and peer review system? I find the obsession with

publishing papers in journals with the highest impact factor unhealthy. I once met a scientist who could recite impact factors to the third decimal point, which is not even statistically significant. Come to think about it, an impact factor is only an average, so why doesn't it come with standard deviation? How can journal X claim that it has the highest impact factor in a given field without demonstrating the difference with the next journal is statistically significant? Besides, impact factors can be quite misleading. A pioneering paper may generate very few citations (at least initially) before the field has caught on. On the other hand, a run of the mill paper on a mainstream subject may generate a substantial number of citations, simply because a vast number of papers are published on the subject. On average, top journals tend to publish more important papers, but that is only a correlation. At the end, a paper has to be judged by what is in it, and not where it is published. A comment on peer review: usually the more prestigious the journal, the more 'mechanistic insight' the reviewers and editors would demand; but what constitutes mechanistic insight can be a rather subjective criterion. I once thought of naming one of the genes we were studying *mechanistic insight* (*mei*) so the reviewers/editors cannot say that we did not provide mechanistic insight.

What is your greatest ambition in research? I hope that our best work is still ahead of us. My current passion is dendrite development, which is I think a very aesthetically pleasing problem. We hope to be able to make progress so that a few years from now, we can take some of Cajal's beautiful drawings and begin to explain how different neurons acquire their distinctive shapes.

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A whiff of home

Researchers have revealed numerous cues animals use for quite extraordinary migrations across land and sea from stellar, solar, magnetic and other environmental sources. But cues used by one animal to locate one tiny island in the middle of the Atlantic Ocean from thousands of kilometres away, a puzzle noted by Darwin, has remained unsolved.

But new work by a team of researchers from the University of Wales Swansea, Lund University and the University of Pisa reported in the biology letters of the Proceedings of the Royal Society (published online), suggests that the island's smell may provide vital cues for homebound turtles.

Green turtles (*Chelonia mydas*) migrate from their grazing grounds off the coast of Brazil in their thousands to the beaches of Ascension Island more than 2,000 kilometres away to mate and lay eggs. Efforts to determine how they achieve this navigational feat have been stymied by the sheer technical difficulties. Adults off Brazil do not come ashore so are hard to locate and individuals only return to Ascension Island once every three or four years. Also the population off Brazil includes individuals that breed at other locations so that captured and tagged individuals may not be those originating from the tiny Atlantic island.

To try to overcome these problems the team captures six individuals who had just laid their first clutch of eggs on the island – they often lay multiple clutches so were likely to wish to return – and placed three 50 kms upwind of the island and three 50 kms downwind. Satellite transmitters were attached to the animals' carapace to study their movements. The team found that the individuals released downwind had no difficulty returning to the island within a few days but those upwind had much more difficulty, with two returning after 10 and 27 days and the third heading back to Brazil after failed efforts to relocate the island.

The authors believe their results suggest that wind-borne smells may be crucial for the turtles' final ability to find home but that other cues may also be necessary for them to head off on the right track first. `Even if they routinely use wind-borne information... for the final approach to the target, it is unknown how they manage to reach this area,' the authors report.

Wind power: Studies suggest that green turtles migrating to beaches on Ascension Island use wind-borne cues to navigate. (Photo: Oxford Scientific Films.)